MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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J. R. HIND, Esq., F.R.S., President, in the Chair.

Edwin Simpson-Baikie, Esq., F.L.S., F.R.G.S., 21 York Terrace, Regent's Park, and United University Club, Pall Mall East; and

Colonel Henry T. Rogers, R.E., Superintendent of Survey, Madras, 24 Bassett Road, Notting Hill, W.:

were balloted for and duly elected Fellows of the Society.

Plan for telegraphing Astronomical Discoveries.

(*From a letter of B. A. Gould to the Secretary of the Smithsonian Institution, 1880, Nov. 8* [*with slight and unimportant modifications*]).

(*Communicated by the President.*)

I inclose the German astronomers' plan for telegraphing the discovery of Comets, Asteroids, &c., across the Atlantic. American observers are affected by it only as concerns the intelligibility of messages *received* by you. The Europeans seem of accord that they can receive most intelligibly messages sent in this way.

If you approve and adopt, please let me know at your early convenience, that the matter may be decided at once.

1. Only one despatch to be sent at a time. This, if for Europe, to be addressed to Prof. Krueger, at the Observatory in Kiel; if for America, to Prof. Baird, Secretary of the Smithsonian Institution. A second telegram to be sent as soon as a second observation has been obtained. Thus only two despatches in all.

2. All numerical data to be expressed by a word for each figure, without regard to its value as dependent upon position. Thus 58 to be telegraphed as "five-eight," 12 as "one-two," 30 as "three-zero," &c.

3. All places which would ordinarily be left vacant, when the number is written in figures must be filled up by the word "nought," or "zero." Thus for R. A.$=1^h\ 5^m{\cdot}3$ would be written "zero-one-zero-five-three," or for $19^h\ 3^m$ the despatch would be sent "one-nine-zero-three-zero" (the tenths of minutes being given for Right Ascensions, but not for other data, as by § 5).

4. The time of observation to be given in round hours of Greenwich mean time, preceded by day and month, and never in any other way. Thus an observation made on Oct. 12 at 13^h Greenwich mean time would be announced "One-two-October-one-three;" or if on Nov. 3 at $7^h\ 15^m$ Greenwich mean time, as "Three-November-zero-seven." In this way the time of observation, which should be given first of all, will be expressed by the name of the month preceded by one or two words, and followed always by two.

5. Right Ascension to follow the time of observation, and to be given by five words, expressing hours, minutes, and tenths of minutes in time, preceded by the word "ascension."

6. Declinations to be given by four words, expressing degrees and minutes, preceded by the word "North" or "South."

7. Daily motion in R. A. to be given in minutes and tenths of minutes of time, preceded by the word "plus" or "minus."

8. Daily Motion in Decl. to be given in minutes of arc, preceded by the word "North" or "South."

9. Magnitude, if given, to be preceded by the word "mag.," but expressed as an ordinal number.

Thus, in ordinary cases, the essential data will be comprised in twenty-three words, or twenty-five if the magnitude be given.

Example:—

Comet discovered by Swift Oct. 12. Place at 7^h Greenwich mean time R. A.$=2^h\ 23^m{\cdot}7$, Decl.$=+31°\ 2'$. Daily motion in R. A. $1^m{\cdot}2$ decreasing; in Decl. 8′ northward.

The despatch would be:—

comet swift one two october zero seven ascension zero two two three seven north three one zero two minus one two north eight.

Prof. Krueger (appointed editor of the *Astron. Nachrichten*) undertakes to repeat the despatch throughout Europe at the cost of the Kiel Observatory or of the Astronomische Gesellschaft. Prof. Foerster informs me that the Paris Observatory as well as the German ones accept the arrangement. At Greenwich I assured myself of its acceptance there.

On a new Form of "Ghost" Micrometer. By Chas. E. Burton, Esq., and Howard Grubb, Esq.

In the present Note an arrangement for using the images of fixed or moveable lines in the measurement of images, formed by the objective of a telescope microscope or spectroscope, instead of the material lines hitherto generally employed, is described, and several methods of applying them are indicated.

It is well known that the ordinary wire micrometer is unsuitable for the measurement of two classes of objects, namely, planetary disks, and double stars which are near the limit of separability for the object glass employed when direct measurement is attempted. The difficulties attendant on its use in the cases above mentioned are briefly these:—(1) On bringing a material line in contact with the image of a luminous body of sensible magnitude, diffracted light appears on the side of the wire furthest from the image to be measured; (2) If a wire be placed on the image of a star, that image becomes elongated in the direction perpendicular to the wire. Defect (1) renders it extremely difficult to ascertain when a micrometer wire is accurately in contact, *e.g.* with the limb of a planet; and Defect (2), by reason of the interval between the components of an extremely close double becoming filled with light, when the wires are placed on the components, renders it impossible to obtain direct measures of such objects with the required certainty.

The difficulties above indicated have been treated very completely by Professor Kaiser in vol. iii. of the Leyden Observations, pp. 104–5, where he refers with approval to a proposal by Lamont (*Jahrbuch der K. S. bei München*, Seite 187) to employ "ghost" lines for such measurements; and by the Rev. W. R. Dawes, in *Mem. R.A.S.* vol. xxxv. pp. 153 and 161.*

The suggestions of Prof. Kaiser seem never to have been carried into effect.

In the *Proceedings of the Vienna Academy of Sciences* for 1856, vol. xx. p. 253, H. Karl v. Littrow describes a mode of forming a "ghost" of a system of wires applicable and actually applied to a meridional telescope. In this instrument the ghost lines are interrupted at or near their centres by the interposition

* Since this paper was read before the Society we have been enabled to examine Prof. Lamont's original paper, giving details of the construction of a form of ghost micrometer devised by himself, with which numerous observations of the satellites of *Jupiter* and the ring system of *Saturn* were made. The images of the lines (bright) or holes in a diaphragm were reflected by a glass plate nearly perpendicular to the end of the telescope, through which the stellar or planetary light passed. Professor Lamont's intention was (*loc. cit.*) to get rid of the errors of judgment which are almost or quite unavoidable when the extreme points of the line under measurement are concealed by the wires employed for that measurement. He does not mention the still more injurious effects of diffraction referred to by Mr. Dawes, and in paragraph (2) of this Note. Professor Stämpfer also employed a similar arrangement with much success for meridian work (*Sitz. Berichte Wien. Ak.* Bd. 20, Seite 314).

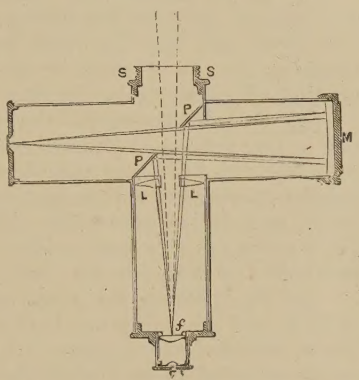
of an opaque bar, and the star is caused to traverse the series of blank spaces thus formed. It does not appear, however, that any attempt was made to adapt this to a position micrometer of an equatoreal instrument.

An instrument somewhat resembling that of Dr. Littrow, and designed for use as a position micrometer, was devised by Mr. G. P. Bidder, and is described by him in the *Monthly Notices* for June, 1874. In this form of ghost micrometer the image of the lines is formed by a unilateral oblique pencil of rays, and it would probably be difficult to avoid loss of definition due to dispersion by the lenses of the eyepiece, as well as to preserve an invariable scale value, on account of the want of a sufficiently rigid connection between the several parts of the instrument. Any movement of the second prism of the optical train, as proposed for the purpose of transferring the system of "ghost" lines to any part of the field, would directly tend to alteration of the scale value and introduce a variable parallax of serious amount.

The instrument now to be described is one of several forms contrived with the object of avoiding the difficulties encountered by those who have hitherto worked at this subject, and of extending the powers of the wire micrometer into the field occupied by the double image micrometer, by rendering the measurement of planetary disks, and close double stars practicable without the production of interference phenomena or the necessity of halving the light of the image, the last being a great hindrance to the use of the double image micrometer in the measurement of faint objects. At the same time the connection between the several parts of the instrument is so rigid and permanent that variation in the scale value will be produced by temperature only, and the corrections thereby necessitated will be of the same order with those already familiar to observers.

The accompanying figure represents the most simple form of the instrument hitherto devised by us, one-third of its actual size. Externally it consists of a pair of tubes, one in the axis of the telescope, and the other at right angles thereto. The first of these tubes is attachable to the draw tube by an ordinary "Tulley" screw at S, and carries at its lower end the eyepiece E. The cross tube carries at one end a "Reticule" or wire frame F, moveable or stationary, according to requirement, and at the other a cell containing a concave silvered mirror M, the radius of curvature of which is slightly greater than the distance between F and M. At the junction of the tubes is mounted (at 45° to F M) a plane silvered mirror P, perforated by an elliptical hole slightly larger than is necessary to allow the pencil of rays from the object glass to pass. Immediately below P is inserted a perforated achromatic lens L, to increase the convergence of the rays from M, and thus diminishing the length of the eyetube while forming an image of F at the focus of the object glass. F being illuminated by a faint external

light, the rays from it pass to M through the hole in P, and, being reflected by M in a cone of smaller angle, are received on the inner edge of P and are thereby diverted into the eye-tube, where they are still further condensed by L and brought to a focus at *f*, the focus of the object glass.



In some cases the lens L may be necessary, and in others it may be found better to use a plane mirror at M and throw the whole work of forming the "ghost" images on the lens L. It is, of course, not necessary that the cross tube should be at *right angles* to the eyetube, and in some cases it may be advisable to use a different arrangement of the parts. If it should prove to be difficult to adapt the instrument to any existing telescope on account of its length (four inches), a Barlow lens can be inserted at S; but some forms of the apparatus have been devised in which there is no cross tube, and the "ghost" images are formed by a perforated concave mirror mounted in a tube capable of being pushed into the draw tube of the telescope. The best form of condensing lens L is under consideration, and it may possibly be found desirable to superachromatise it, to compensate for the undercorrection of the usual eyepieces for lateral pencils.*

In two of its simplest forms, including that represented in the figure, the "ghost" micrometer has been severely tested by one of us, as regards the effect of the images of the lines upon the images of various celestial objects furnished by an equatorially mounted objective of six inches' aperture and six feet focal length. The objects scrutinised for this purpose were the following: lunar details near the terminator and fully enlightened; *Jupiter* and *Saturn* with their satellites; and numerous stars, both single and double. In all cases the result was the same, not the slightest change of form or false light beyond the limb (of the planets) being visible, or even suspected, when the objective images were brought in contact with or were occulted (covered) by the "ghost" lines, powers up to 400 linear being employed whenever definition was suitable. The illumination of the reticules employed was extremely easy, and readily adjusted to suit the brightness of the object observed. It was possible with the direct light of an ordinary candle, unassisted by any optical arrangement for rendering it parallel or convergent, to increase the brightness of the lines sufficiently to make them clearly visible against the brightest parts of the

* Another form has since been devised in which this lens is not required.

Moon's image. The reticules employed were systems of lines cut through an opaque film deposited upon glass.

Suggested forms of "ghost" micrometer, with their applications.

(1) In its most simple form this micrometer will do duty as a transit eyepiece, displaying dark lines on a bright field, or *vice versâ*, and can be fitted with reticules of any kind for mapping groups of stars, or details of the Moon or planets, including of course circles for ring micrometer work.

(2) By substituting for the reticule a wire micrometer with one or two screws, the instrument can be used (*a*) instead of an ordinary uni- or bi-filar micrometer with bright field; or (*b*) as a unifilar micrometer with bright lines cut through an opaque film; or (*c*) as a bifilar micrometer with bright lines, the object lines (at F) being in this case spider webs illuminated by a Wenham paraboloid or other similar contrivance.

(3) By splitting the plane mirror P centrally in the plane M P E, and mounting one half on an axis perpendicular to that plane, attached at its upper end to a lever arm terminating in a sector actuated by an endless screw with divided head, a double set of "ghost" lines might be formed by inclining the two parts of the plane P to one another by a movement of the arm and screw. One of these sets of lines would be stationary, while the other moveable set would have its movement registered by the turns and parts of a turn of the endless screw. Of course the lines might be either dark on a bright field, or bright on a dark field, the latter being best, as in the former case the lines would not be black, but would be of half the brightness of the field. We have experimentally proved that the definition of the lines is unaffected by stopping off half the aperture of M or P in a direction perpendicular to them, and that their brightness does not sensibly diminish for a considerable distance past the centre of the field.

(4) By substituting diaphragms pierced with small circular apertures illuminated by polarised light variable in intensity and colour, this micrometer can be made available for photometric and colour comparisons of stars after the method of Zöllner. Not only can the field, in the case of bright field illumination, be illuminated with light of any desired tint, but if bright lines are employed they can be coloured as the observer may prefer, for there is always a superabundance of light which is yet completely under control. The lines can be shown "broken" or continuous, as may be desired. The first-mentioned arrangement may be advantageous under some circumstances.

A useful modification of the reticule mentioned in a former section would be a bright ring (photographed), divided into degrees and nearly of the same diameter as the field, which would assist materially in estimation of position angle when the faintness of the object viewed rendered *measurements* impossible.*

* This suggestion has been anticipated by Dr. Lamont. *Vide supra.*

Lastly, when a slight loss of light is of no importance, a glass plate with plane and parallel sides, the preparation of which is a matter of no difficulty in the present state of practical optics, may be substituted for the plane mirror P, with the advantage of securing a large field, which may be surveyed by a low power eye-piece, or by one of high power so mounted as to be traversable over the whole field.

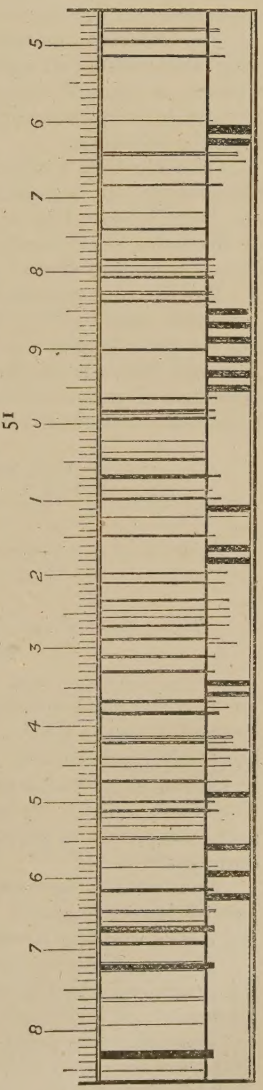
On the Spectrum of a Sun-Spot observed at the Royal Observatory, Greenwich, 1880, Nov. 27 and 30.

(*Communicated by the Astronomer Royal.*)

Amongst the spectra of Sun-spots which have been examined at Greenwich, that of the fine spot which was first seen near the east limb on Nov. 25 appears to be quite unique. When examined on Nov. 27, the most striking feature in the spectrum was a number of strong dark lines between *b* and F, to which there appeared to be nothing corresponding in the ordinary spectrum of the photosphere. Under favourable circumstances, however, faint lines were detected in the solar spectrum corresponding to all, or nearly all, of these dark lines in the spot, but none of these are represented either in Kirchhoff's or Ångström's maps, and they do not appear to correspond to any known element. The dark lines in the spot were fully as broad as b_1 or b_2, being about one tenth-metre in breadth, and somewhat fuzzy at the edges.

The following are their wave-lengths, as derived from careful comparison with the lines in Ångström's map :—

5162·3	5118·2	5088·8
5159·7	5116·5	5086·9
5155·9	5111·0	5084·9
5148·9	5095·2	5062·7
5135·3	5093·4	5061·1
5134·0	5091·2	



The lines at 5129·4, 5112·1, and 5142·2 were much broadened in the spot; D and F were about one-third as broad again, and upwards of 300 lines between D and F were noted as being more or less affected.

On Nov. 30 the above-mentioned bands were not so marked, but the C and F lines were much broader (about doubled in breadth), and many lines between B and *b* were very much darkened and broadened. The *b* lines were hardly affected.

The observations were made by Mr. Christie and Mr. Maunder with the half-prism spectroscope, mounted on the Great Equatoreal. A train of two prisms, giving a dispersion of about 80° from A to H, was used, with a magnifying power of 10 on the viewing telescope.

On 1880 Oct. 1 a strong dark line at W. L. 5146·3, not corresponding to any line in Ångström's map, was seen in the spectrum of two Sun-spots. This dark line, though resembling in character those mentioned above, does not correspond in position with any of them.

The figure represents a portion of Ångström's map of the solar spectrum near *b*, with the spectrum of the Sun-spot below, showing the dark bands. In the case of the other lines the length represents the amount by which the line is broadened—full length corresponding to a line which is doubled in breadth; half length, to one of which the breadth is increased by one half; and so on.

Royal Observatory, Greenwich,
1880, *December* 7.

Telegraphic Determination of the Longitude of Shanghai.

By Dr. L. S. Little.

The completion of the submarine telegraph cable to Shanghai suggested to me that it was desirable to obtain the value of the longitude by its means. Shanghai being the most important port in China, an accurate determination is of importance to navigation, even if I did not require it for my own Observatory work, and to correct the value I had obtained by occultations and Moon culminations.

Shanghai is in telegraphic communication with Europe by two routes—a southern, *viâ* Hongkong and India, and a northern, *viâ* Japan and the Russian Empire.

The positions of several points in Japan were determined telegraphically by the various parties sent out to observe the last transit of *Venus*.

One of these points was the telegraph station at Nagasaki, and I am indebted to Prof. A. Hall, of the Naval Observatory at Washington, for the longitude of that point.

The telegraph station at Nagasaki is connected with that of Shanghai by

426	miles	submarine cable	Nagasaki to Gutzlaff,
57	„	„	Gutzlaff to Woosung,
9	„	land line	Woosung to Shanghai.

In 1875 Vice-Admiral Ryder, Commander-in-Chief on the China station, directed Commander Dawson, of H.M.S. "Dwarf," with Lieutenants Bedford and Greaves, officers specially charged with surveying duties, to carry out the work at Nagasaki.

Lieut. Dreyer, of the Royal Danish Navy, Director of the Great Northern Telegraph Company in Shanghai, kindly placed the line at our disposal after ten o'clock at night without relays or breaks at Woosung or Gutzlaff.

The bell of Wheatstone's Morse instrument and the key of Wheatstone's (double current) instrument were used.

At Nagasaki local time was obtained by the observation of equal altitudes of the Sun by two observers independently. The observations consisted of from three to four sets of five each, both morning and afternoon, and were made in the yard of the telegraph office, where a mean-solar-time chronometer was kept during the week the work was going on.

At Nagasaki the weather was fine throughout.

At Shanghai local sidereal time was obtained by means of a fixed transit instrument and a marine sidereal chronometer beating half seconds. The Observatory is situate about a third of a mile from the telegraph office, and the chronometer was carried to and fro by hand.

Corrections of Nagasaki Mean Solar Chronometer, Dent No. 1505.

Date 1875.	Chronometer fast of Mean Time at Noon Bedford.	Greaves.	Mean.	Hourly Rate.	Difference between the two Observers.
	h m s	h m s	h m s	s	s
Nov. 1	3 6 32·738	3 6 34·087	3 6 33·250	+0·2275	1·149
2	27·535	28·260	27·791	+0·2965	0·725
3	20·426	21·427	20·676	+0·2594	1·001
4	14·310	14·590	14·450		0·280

It will be noticed that the difference between the two observers is much less on the fourth day, and this gives greater weight to the correction for that day, and to the longitude deduced from it.

Corrections of Shanghai Sidereal Chronometer, Mierendorff No. 10.

Day and Hour. 1875.		Correction.	Hourly Rate.
d	h	m s	s
Nov. 1	21·0	+12 27·06	
			+0·046
2	2·4	28·42	
			+0·077
3	21·70	29·91	
			−0·107
3	1·45	29·51	

In consequence of unfavourable weather, it was not possible to determine the instrumental errors on the first two nights as accurately as could be wished, but on the third night the errors were obtained more accurately.

The resulting longitudes determined on the three days of observation are exhibited in the following table, which gives the comparison of the values of the difference of longitude obtained from coincidences with that obtained by ten-second signals, omitting minutes.

Date 1875.	Wght.	Shanghai to Nagasaki			Nagasaki to Shanghai			Means adopted.		
		Coincid. 10ˢ sig.		Diff.	Coincid. 10ˢ sig.		Diff.	Coincid. 10ˢ sig.		Diff.
		s	s	s	s	s	s	s	s	s
Nov. 1	1	36·68	36·59	−·19	35·81	35·73	−·08	36·24	36·16	−·08
2	2	36·20	35·95	−·25	35·36	35·17	−·19	35·78	34·56	−·22
3	5	36·41	36·35	−·06	35·67	35·52	−·15	36·04	35·94	−·10
Means		36·39	36·28	−·12	35·61	35·52	−·15	36·00	35·87	−·13

This table shows that Shanghai observed systematically too late and Nagasaki too early by about the same amount, one-eighth of a second, assuming that in coincidences there is no correction required for personal equation.

The wave and armature time varies on the different days and with the different sets of signals from $0^s{\cdot}37$ to $0^s{\cdot}44$.

From the equations of condition formed from the wave and armature times and relative lengths of the cables given in Commander Green's telegraphic determinations of longitudes in the West Indies, the following values were found:—

x = armature time = $0^s{\cdot}2971$,

y = wave time for 100 miles = $0^s{\cdot}0175$ = 5700 miles in a second.

Applying this value of x to the mean wave and armature time obtained as above, $y = 0^s{\cdot}0186$, or nearly the same value as that found by Commander Green.

The following table gives the resulting longitude of Shanghai East of Greenwich, as determined from these observations :—

	h	m	s
Wladiwostok, East of Greenwich, by cable	−8	47	30·91
Nagasaki, West of Wladiwostok, by cable	+	8	1·92
Nagasaki (telegraph office) East of Greenwich	−8	39	28·99
Shanghai (Observatory), West of Nagasaki	+	33	36·00
Shanghai (Observatory), East of Greenwich	−8	5	52·99

The latitude of the Observatory as determined by transits of stars on the prime vertical is

N. 31° 14′ 10″23.

Ephemeris for finding the Positions of the Satellites of Uranus, 1881. By A. Marth, Esq.

About the beginning of October 1881 the Earth will pass through the planes of the orbits of the satellites of *Uranus*, from the side on which the satellites appear to move in the direction of *de*creasing position-angles to the other side; it will repass the planes about the end of March 1882, and will, about the middle of July 1882, finally pass to the side on which the satellites appear to move in the direction of *in*creasing position-angles, and on which it will remain till the year 1923. The apparitions of the planet in 1881, 1882, and 1883 will consequently be most favourable for the determination of the planes of the satellites' orbits. The most important measurements for the purpose are those of the position-angles when the satellites are near their greatest elongations, and when they are very near the planet; and to secure these observations it may be advisable, in case there is not ample opportunity for observing, to dispense with getting exact measurements of the distances, and to devote the available time to obtaining greater accuracy in the position-angles. It has not yet become known how near to the planet the satellites may be observed with some modern instruments.

The angle of position p_o of the major axes, the major and minor semi-axes a and b of the apparent ellipses described by the satellites, and the latitude of the Earth above the assumed plane of their orbits, are the following :—

Greenwich Noon.	p_0	Ariel. a	Ariel. b	Umbriel. a	Umbriel. b	Titania. a	Titania. b	Oberon. a	Oberon. b	Lat. of Earth.
1881.	°	′	″	″	″	″	″	″	″	°
Jan. 9	14·75	14·95	0·57	20·82	0·80	34·15	1·31	45·67	1·75	−2·20
19	14·72	15·06	0·64	20·98	0·89	34·41	1·46	46·01	1·95	2·43
29	14·68	15·15	0·72	21·11	1·01	34·62	1·66	46·30	2·21	2·74
Feb. 8	14·63	15·22	0·82	21·21	1·15	34·79	1·89	46·52	2·52	3·11
18	14·58	15·27	0·93	21·27	1·30	34·89	2·14	46·66	2·86	3·51
28	14·52	15·29	1·05	21·30	1·46	34·94	2·40	46·72	3·21	3·94
March 10	14·46	15·28	1·16	21·29	1·62	34·92	2·66	46·70	3·56	4·37
20	14·40	15·25	1·27	21·25	1·77	34·85	2·91	46·60	3·89	4·79
30	14·34	15·19	1·37	21·16	1·91	34·71	3·13	46·42	4·18	5·17
April 9	14·29	15·11	1·45	21·05	1·02	34·53	3·31	46·17	4·43	5·50
19	14·25	15·01	1·51	20·91	2·10	34·30	3·45	45·87	4·62	5·77
29	14·22	14·90	1·55	20·75	2·16	34·04	3·54	45·52	4·73	5·97
May 9	14·20	14·77	1·56	20·58	2·18	33·75	3·58	45·14	4·78	6·08
19	14·20	14·64	1·56	20·40	2·17	33·45	3·56	44·74	4·76	6·10
29	14·21	14·51	1·53	20·21	2·13	33·15	3·49	44·33	4·67	6·04

Longitudes of the satellites in their orbits reckoned from the points where they are at their greatest northern elongations :—

Greenwich Noon. 1881.		Ariel. Long.	Diff.	Umbriel. Long.	Diff.	Titania. Long.	Diff.	Oberon. Long.	Diff.
		°	°	°	°	°	°	°	°
Jan.	9	228·23	1428·43	218·61	868·71	15·11	413·50	9·39	267·37
	19	276·66	·39	7·32	·70	68·61	·49	276·76	·36
	29	265·05	·37	156·02	·67	122·10	·48	184·12	·36
Feb.	8	253·42	·34	304·69	·66	175·58	·47	91·48	·34
	18	241·76	·31	93·35	·64	229·05	·45	358·82	·34
	28	230·07	·29	241·99	·62	282·50	·45	266·16	·33
March	10	218·36	·27	30·61	·61	335·95	·45	173·49	·33
	20	206·63	·25	179·22	·60	29·40	·45	80·82	·33
	30	194·88	·23	327·82	·60	82·85	·45	348·15	·34
April	9	183·11	·23	116·42	·60	136·30	·45	255·49	·35
	19	171·34	·22	265·02	·60	189·75	·46	162·84	·35
	29	159·56	·22	53·62	·61	243·21	·47	70·19	·36
May	9	147·78	·23	202·23	·61	296·68	·48	337·55	·37
	19	136·01	·24	350·84	·62	360·16	·48	244·92	·39
	29	124·25		139·46		43·64		152·31	

These values are to be interpolated for the times for which the positions of the satellites are required. The position-angles p and distances s are then found by means of the formulæ—

$$s \cos (p_0 - p) = a \cos \text{long.}$$
$$s \sin (p_0 - p) = b \sin \text{long.}$$

The satellites move in the direction of decreasing position-angles, and will be at their greatest elongations ("N" in posit. p_0 and "S" in posit. $p_0 + 180°$) and at their superior and inferior conjunctions with the planet ("sup." in posit. $p_0 + 90$ and "inf." in posit. $p_0 + 270°$) at the following hours, Greenwich mean time :—

Ariel.

1881	N.		S.			N.		S.	
	d	h	d	h		d	h	d	h
Jan.	9	12·1	10	18·3	March	23	14·3	24	20·5
	12	0·5	13	6·8		26	2·8	27	9·0
	14	13·0	15	19·3		28	15·3	29	21·5
	17	1·5	18	7·8		31	3·7		
	19	14·0	20	20·2	April			1	10·0
	22	2·5	23	8·7		2	16·2	3	22·5
	24	15·0	25	21·2		5	4·7	6	11·0
	27	3·5	28	9·7		7	17 2	8	23·5
	29	16·0	30	22·2		10	5·7	11	12·0
Feb.	1	4·4	2	10·7		12	18·2	14	0·5
	3	16·9	4	23·2		15	6·7	16	13·0
	6	5·4	7	11·7		17	19·2	19	1·5
	8	17·9	10	0·2		20	7·7	21	14·0
	11	6·4	12	12·6		22	20·2	24	2·4
	13	18·9	15	1·1		25	8·7	26	14·9
	16	7·4	17	13·6		27	21·2	29	3·4
	18	19·9	20	2·1		30	9·7		
	21	8·4	22	14·6	May			1	15·9
	23	20·9	25	3·1		2	22·2	4	4·4
	26	9·3	27	15·6		5	10·7	6	16·9
	28	21·8				7	23·2	9	5·4
March			2	4·1		10	11·7	11	17·9
	3	10·3	4	16·6		13	0·2	14	6·4
	5	22·8	7	5·1		15	12·6	16	18·9
	8	11·3	9	17·6		18	1·1	19	7·4
	10	23·8	12	6·0		20	13·6	21	19·9
	13	12·3	14	18·5		23	2·1	24	8·4
	16	0·8	17	7·0		25	14·6	26	20·9
	18	13·3	19	19·5		28	3·1	29	9·4
	21	1·8	22	8·0					

Umbriel.

1881	N.		S.	
	d	h	d	h
Jan.	6	11·7	8	13·3
	10	15·1	12	16·8
	14	18·5	16	20·2
	18	22·0	20	23·7
	23	1·4	25	3·2
	27	4·9	29	6·6
	31	8·4		
Feb.			2	10·1
	4	11·8	6	13·5
	8	15·3	10	17·0
	12	18·7	14	20·5
	16	22·2	18	23·9
	21	1·7	23	3·4
	25	5·1	27	6·9
March	1	8·6	3	10·3
	5	12·1	7	13·8
	9	15·5	11	17·3
	13	19·0	15	20·7
	17	22·5	20	0·2

	N.		S.	
	d	h	d	h
March	22	1·9	24	3·7
	26	5·4	28	7·2
	30	8·9		
April			1	10·6
	3	12·4	5	14·1
	7	15·8	9	17·6
	11	19·3	13	21·0
	15	22·8	18	0·5
	20	2·2	22	4·0
	24	5·7	26	7·4
	28	9·2	30	10·9
May	2	12·7	4	14·4
	6	16·1	8	17·9
	10	19·6	12	21·3
	14	23·1	17	0·8
	19	2·5	21	4·3
	23	6·0	25	7·7
	27	9·5	29	11·2

Titania.

	N. elong.		Inf. conj.		S. elong.		Sup. conj.	
	h		h		h		h	
Jan.	8	15·2	Jan. 10	19·5	Jan. 12	23·7	Jan. 15	3·9
	17	8·2	19	12·4	21	16·6	23	20·9
	26	1·1	28	5·4	30	9·6	Feb. 1	13·8
Feb.	3	18·1	Feb. 5	22·3	Feb. 8	2·6	10	6·8
	12	11·0	14	15·3	16	19·5	18	23·8
	21	4·0	23	8·3	25	12·5	27	16·7
March	1	21·0	March 4	1·2	March 6	5·5	March 8	9·7
	10	14·0	12	18·2	14	22·4	17	2·7
	19	6·9	21	11·2	23	15·4	25	19·7
	27	23·9	30	4·2	April 1	8·4	April 3	2·6
April	5	16·9	April 7	21·1	10	1·4	12	5·6
	14	9·9	16	14·1	18	18·3	20	22·6
	23	2·8	25	7·1	27	11·3	29	15·5
May	1	19·8	May 4	0·0	May 6	4·3	May 8	8·5
	10	12·8	12	17·0	14	21·2	17	1·5
	19	5·7	21	10·0	23	14·2	25	18·4
	27	22·7	30	2·9				

Oberon.

	N. elong.		Inf. conj.		S. elong.		Sup. conj.
	h		h		h		h
Jan. 8	15·6	Jan. 12	0·4	Jan. 15	9·1	Jan. 18	17·9
22	2·7	25	11·5	28	20·3	Feb. 1	5·1
Feb. 4	13·9	Feb. 7	22·7	Feb. 11	7·5	14	16·3
18	1·1	21	9·9	24	18·7	28	3·4
March 3	12·2	March 6	21·0	March 10	5·8	March 13	14·6
16	23·4	20	8·2	23	17·0	27	1·8
30	10·6	April 2	19·4	April 6	4·2	April 9	13·0
April 12	21·8	16	6·6	19	15·4	23	0·2
26	9·0	29	17·8	May 3	2·6	May 6	11·4
May 9	20·2	May 13	4·9	16	13·7	19	22·5
23	7·4	26	16·2	30	0·9		

Observations of Meteors, Nov. 25–28, 1880.
By W. F. Denning, Esq.

Observations for the Andromedes or meteors of Biela's Comet were commenced here on Nov. 25, and partly continued until Nov. 29, but there were no active indications of the showers' return.

On Nov. 25 the sky was watched at intervals, but meteors were somewhat scarce. Only one was noted between $5^h\ 45^m$ and $6^h\ 15^m$, and that belonged to a radiant near *η Ursæ Majoris*. At $6^h\ 42^m$ a fine slowish meteor, with a thick spark-train, was seen shooting between *α Cephei* and *δ Cygni*, and its path conformed exactly to the radiant of the Andromedes. Several other meteors were recorded, but they were obviously directed from showers in *Taurus*, *Auriga*, and *Draco*.

On Nov. 26 a very remarkable meteor was seen at $6^h\ 14\frac{1}{2}^m$. When first observed it was passing 5° N.E. of *ι Ceti*, with a singularly slow motion, and its disappearance occurred 2° below *ζ-η* Aquarii. Length of path 31°. Directly I saw the meteor I began to count, and had enumerated 52 at its extinction. This I found by comparison with a seconds watch immediately afterwards to be equivalent to 15 seconds. The entire duration of the meteor must have considerably exceeded this, for it had already traversed a section of its course when it first came under my observation. It was the slowest meteor ever seen here. The radiant point was probably in *Eridanus* on the E.S.E. horizon. Only one meteor was observed during the evening with a path conforming with the radiant of the Andromedes, but observations were not of long duration.

On Nov. 27 the sky was observed during about three hours before $13^h\ 20^m$, and 24 meteors registered, but no Andromedes.

Though on the two preceding nights this shower may have been very feebly visible, there was not, on the 27th, the slightest evidence of its return. A few fine meteors were seen belonging to two well-known streams of Taurids (at 63°+21° and 78°+24°): indeed, one-half the total number observed belonged to these showers, the former of which appeared to be the richer of the pair. I had never seen the Taurids I. so late before, though in 1876, Nov. 20, the shower was well observed here as a prominent display of bright meteors from the point 62°+22½°; and in the *Monthly Notices*, vol. xxxix. p. 23, I had pointed out the probable endurance of the shower until Dec. 6. As to the companion shower near *β–ζ Tauri* noticed this year, that also was well observed in 1876, both at the end of November and early in December, when 26 of its meteors were recorded. It was reobserved in 1877 by Messrs. Corder and Sawyer and the writer, and in 1878 Mr. Sawyer, during a series of observations for the Andromedes, found this the chief shower visible, and determined its radiant at 81°+21° (9 meteors). Mr. Greg designated this stream the Taurids II., in order to distinguish it from its rival and apparently contemporary shower near *ε Tauri* (the Taurids I.). It evidently forms a display above the average strength, and endures from Nov. 22 to Dec. 12 without apparent cessation. The following are the observations of this stream which have been made since 1876:—

		α	δ		
		°	°		
1876	Nov. 22–Dec. 8	80	+23	26 ↓ *s*	Denning,
1877	Nov. 25–Dec. 13	81	+25	6 ↓ *s*	Denning,
1876–9	December	78	+23	20 ↓ *s*	Corder,
1877	Nov. 30–Dec. 9	80	+22	10 ↓ *s*	Sawyer,
1878	Nov. 26–29	81	+21	9 ↓ *s*	Sawyer,
1879	November 27	78	+24	5 ↓ 's	Denning,
	Dec. 7–13	83	+23	27 ↓ *s*	Denning (Foreign Obs.)
[1869	December 11	82·9	+22·9	stationary ↓	Bartel (Brünn)],

giving an average position at 80°·5+23°·0 from the eight observations.

The largest meteor seen this year on Nov. 27 was a Taurid at 9^h 2^m brighter than *Jupiter*. It traversed nearly the whole of its path as an ordinary shooting-star less than first magnitude, but suddenly burst out, at the end-point, into a brilliant flash, where it left a short streak of about ½° for some 5 seconds. At 12^h 29^m a stationary meteor was noted in 78°+24°. It was evidently approaching the observer in a sinuous course, so ♌, and appeared in the diverging point of the Taurids II.

On Nov. 28 observations were renewed, but there were no Andromedes, though the showers of Taurids continued in active progress. The following were the most remarkable shooting-stars registered on the several nights of observation:—

Paths of Shooting Stars. November 25-28, 1880.

No.	Date. 1880	G. M. T.	Mag.	Path from α δ	Path to α δ	Length of Path.	Notes.	Radiant Point.
		h m		° °	° °			
1	Nov. 25	6 42	1	319+58	289+51½	19	1½ sec. Bright train.	γ Androm.
2	26	6 14½	2	6− 7½	335− 4	31	15 sec.; yellow; very slow. Died out in a faint spark.	Eridanus
3	26	6 52	2	27+54	26+60	6	Slow.	? γ Androm.
4	27	7 36	1	21− 9	9−17	14	3 sec.; very slow; sparks.	Taurid I. or II.
5	27	9 2	> 1	57+67	38+82	15½	Bright flash and streak at the end.	Taurid I.
6	27	11 13	1	27+16	14+12	13	Slowish.	Taurid I.
7	27	12 16	2	15+24	350+20	23½	Slow.	Taurid I.
8	27	12 29	2	18+ 5	7− 2	13	Bright train; on horizon.	Taurid I. or II.
9	27	12 29	3	78+24			Stationary; 1½ sec.	Taurid II.
10	27	12 38	2	54− 9	52−16	7	Short and swift	Taurid I.
11	27	12 45	1	86+56	95+70	14½	Not swift; train.	Taurid II.
12	27	13 0	> 1	211+41	229+41½	13	Slowish; on horizon.	?
13	27	13 15	3	49+25	45+10½	15	Very very swift.	η Ursæ.
14	27	13 20	4	54+16	49+14	5	Slowish. Foreshortened near radiant.	Taurid I. or II.
15	28	7 33	1	336+55	290+39	34	Slowish.	Taurid I.
16	28	12 6	2	343+36	333+32	9	Rather swift.	Taurid II.

At $10^h\ 59^m$, Nov. 27, two small meteors appeared simultaneously, in the Lynx, with paths intersecting thus :—

The Meteors of November 14, 1880, *at the Observatory of Moncalieri* (*Italy*). By Signor P. F. Denza.

Fogs and clouds have almost completely prevented the observations we intended making at this Observatory on the nights which elapsed from the 12th to the 15th inst., in order to follow the phenomena which at this period usually present themselves.

On the morning of the 14th, the clouds and afterwards fogs began gradually to dissipate themselves at about 4 o'clock A.M., and by a short time before 5 the sky got clear and favourable enough to make some good observations.

Then we began to work, and from 5 to a quarter to 6—that is, until the increasing daylight obliged us to withdraw—we paid attention to the exploring of the sky.

The results we obtained in so short a time were very satisfactory. Four observers reckoned 37 meteors in three quarters of an hour, making about fifty meteors an hour—that is, more than twelve meteors for each observer, while last year we reached but four or six at the utmost.

More than the third of the observed meteors—that is, thirteen—belonged to the groups of the Leonids.

They all radiated from the celestial region placed in the scythe of *Leo*, the middle position of which is—

$$\text{R. A.} = 147^\circ, \qquad \text{N. Decl.} = 23^\circ,$$

which is almost the same position the English observers found last year—that is, that one of the usual point of radiation of the great current of the Leonids.

Another well-defined shower was observed from *Ursa Major*. Seven meteors accurately determined gave for the radiant of this shower the point—

$$\text{R.A.} = 157^\circ, \qquad \text{N. Decl.} = 46^\circ,$$

which is found near the stars λ and μ of *Ursa Major*.

The meteors of these two groups, and especially those of *Leo*, were amongst all the most beautiful. Of the thirteen Leonids, in fact, two were superior to the first magnitude, two of the first, four of the second, four of the third, and only one of the fourth magnitude.

And amongst the seven of the second group, one surpassed the first magnitude, two were of the second magnitude, and the remaining four of the third or fourth magnitude. No one of the remaining meteors reached the first magnitude.

In the Leonids the red and bluish colour predominated; the colours of the others varied between white and blue.

Several of the first were followed by a luminous ray, and almost all were seen as usual in groups of two or more at a time.

Amongst all the meteors we observed, the most splendid was

the Leonid which appeared at $5^h\ 23^m$ in the constellation of *Hydra*. Its nucleus, larger than *Jupiter*, was refulgent with a bluish light, and left behind it a bright streak of the same colour. The extremes of its course in the celestial region were—

At the beginning	R. A. = 131°,	S. Decl. = 3°,
At the end	R. A. = 124,	S. Decl. = 18.

Our observations, although they were few and of a short duration, nevertheless confirm those made last year in some places in England and in America, and show that the Leonids have in these late years acquired a greater vigour, and that we have thus traversed a space of the meteor current which gave us the grand results of the years 1866–68. The other Italian observations, which have not yet reached us, might perhaps give more weight to our conclusions.

The zodiacal light presented itself with great splendour to the East, on the clear horizon, rising up with its vertex as far as the tail of *Leo*.

Moncalieri Observatory,
1880, *December* 5.

The Leonids, 1880.

(*Extract of a letter from Mr. E. F. Sawyer, Cambridgeport, Mass., to Mr. Denning.*)

"I was on the look-out for Leonids on Nov. 11 and 12 (the 13th and 14th being overcast), and observed 12 as follows:—

"Nov. 11.—Watched from $14^h\ 30^m$ to $16^h\ 30^m$ (two hours), with perfectly clear sky, and recorded 14 meteors, of which 6 were Leonids.

"Nov. 12.—An hour's watch only was obtained in a sky more than half overcast, from $16^h\ 15^m$ to $17^h\ 15^m$; 8 meteors were noted, 6 being Leonids. The rate was at least 15 Leonids per hour, in a clear sky, for one observer. My centre of observation was in *Leo*, and my view was restricted to about one-sixth of the visible heavens.

"The Leonids were bright, 1 > 1st mag. and 2 = 1st. mag. They were short, rapid meteors, with streaks as usual.

"I have received the following note from Mr. Isaac Sharples, of Haverford College Observatory, State of Penn., U.S., regarding this shower. He says, 'Between $3^h\ 15^m$ and $4^h\ 15^m$ A.M. Nov. 13, Phil. mean time, 52 meteors were observed and mapped, of which 28 were Leonids. Four of them were as bright as 1st mag. stars, but the majority were faint. Many left streaks behind them. At the end of the watch, when the sky became cloudy, meteors were coming at the rate of two per minute. There were four observers.'

"This would certainly show that the Leonid meteor-shower was well sustained this year, and doubtless represents the perihelion passage of the minor cluster of 1847."

Note with respect to the Rate of Motion of Gaseous Matter projected from the Sun. By A. C. Ranyard, Esq.

Prof. Young, in the November number of the *American Journal of Science*, describes an exceptionally large prominence which he observed on the morning of October 7 last. He states that at 11 A.M. he saw a bright horn on the S.E. limb of the Sun. "When first seen, it was about 3′ or 4′ in elevation, but it rapidly stretched up and before noon reached a measured altitude of over 13′ (350,000 miles+) above the Sun's limb. It faded away and disappeared about $12^h\ 30^m$. It was brightest about $11^h\ 30^m$ with an altitude of about 8′."

If we suppose that the outburst was directed vertically upwards from a part of the Sun's surface near to the solar limb, so that there was no foreshortening, this observation would prove that the gaseous matter of the prominence was carried from a height of about 212,500 miles above the photosphere to a height of over 350,000 miles in less than thirty minutes. Comparing the rate of upward motion of the gaseous matter of the prominence with the velocity of a projectile thrown vertically upwards in free space to a similar height above the Sun's limb, we find that there is ample evidence to show that the matter of the prominence must have encountered considerable resistance, which caused it to pass over the distance traversed more rapidly than it would if it had moved freely under the action of solar gravity in empty space. Thus, since the time occupied by a projectile in passing upward from any level to the summit of its trajectory is equal to the time occupied by a body in falling under the influence of gravity from the higher to the lower level, we have —taking the Sun's radius as 425,000 miles, and the value of gravity at the level of the photosphere as ·169, where a mile is the unit of length and a second the unit of time—

$$\frac{d^2x}{dt^2} = -\frac{\cdot 169 \times (425000)^2}{x^2},$$

where x is the distance in miles of the falling body from the Sun's centre, and

$$\left(\frac{dx}{dt}\right)^2 = V^2 = \frac{2 \times \cdot 169 \times (425000)^2}{x} + c.$$

If the body drop from a height a miles above the Sun's centre, we have $V=0$ when $x=a$;

$$\therefore c = -\frac{\cdot 338 \times (425000)^2}{x},$$

and

$$\left(\frac{dx}{dt}\right)^2 = \cdot 338 \times (425000)^2 \left(\frac{1}{x} - \frac{1}{a}\right);$$

$$\therefore 425000 \sqrt{\frac{\cdot 338}{a}} \frac{dt}{dx} = \frac{x}{\sqrt{ax - x^2}};$$

which gives

$$425000\sqrt{\frac{\cdot 338}{a}}t = \sqrt{ax-x^2}-\frac{a}{2}\sin^{-1}\frac{2x-a}{a}+c;$$

when $t=0$, $x=a$

$$\therefore c = \frac{a}{2}\frac{\pi}{2};$$

hence we have

$$425000\sqrt{\frac{\cdot 338}{a}}.t = \sqrt{ax-x^2}+\frac{a}{2}\cos^{-1}\frac{2x-a}{a};$$

and for the time of falling from a height of 775,000 miles to a height of 637,500 miles above the Sun's centre we have

$$425000\sqrt{\frac{\cdot 338}{775000}}.t = \sqrt{(137500)(637500)}+387500\cos^{-1}\frac{500000}{775000},$$

which gives

$$t = 2256 \text{ sec}$$
$$= 37^{m}\ 36^{s}.$$

Hence the matter of the prominence must have passed over the lower part of its course with a considerably greater velocity than would have been necessary to carry a projectile unimpeded by a resisting medium to the observed altitude above the photosphere attained by the prominence matter. It seems evident from Prof. Young's description that the matter of the prominence was not carried in a non-luminous condition to an altitude which exceeded the observed altitude, for the gaseous matter composing the higher part of the prominence seems to have been so brightly incandescent that it was more than half an hour in fading from view.

But the observed altitude may have corresponded to a much greater height above the photosphere than 350,000 miles, for the prominence may have been thrown up from a part of the Sun on this side or on the other side of the limb. The calculated time consequently corresponds to a minimum estimate of the time which would have been occupied by a projectile unimpeded by a resisting atmosphere in traversing the space between the observed altitudes.

If on a future occasion the time which the erupted matter occupies in attaining various altitudes can be carefully observed, some valuable deductions may no doubt be drawn with respect to the distribution of the resisting medium which observations made during total eclipses had already shown to exist in the region of the Corona.

The photographs taken during the Eclipse of 1871 show that the tree-like and curving structures observable on that occasion were more contorted in the lower than in the higher regions of

the Corona, but the evidence with respect to the structures which have been visible in other coronas is not sufficiently full to allow of similar generalisations.

The fact that the light derived from the bright structures of the Corona as observed during total eclipses is strongly polarised shows that the brightness of the structures is not chiefly due to incandescent gas. It seems probable that the erupted matter must be quickly precipitated by the cold of space, leaving clouds of dust, which disperses the light derived from the photosphere. The strong polarisation of the light from the bright coronal structures seems to show that the precipitation gives rise to particles which are small compared with the wave-length of light. It is possible (as has frequently been suggested) that showers of metallic rain may also be produced, but the greater part of the light of the structures is evidently due to the light dispersed by the small non-incandescent particles.

Note on the Length of the Pendulum observed by De l'Isle de la Croyère at Archangel in 1728. By Major J. Herschel, R.E., F.R.S.

The observations at Archangel were made by Louis de l'Isle, brother of the geographer, William, and were recounted and communicated to the Academy of St. Petersburg by another brother, Joseph Nicholas, the astronomer, some years later—*i.e.* in 1735.

Their position in the series of pendulum observations should have made them of great importance, but two circumstances conspired to make them valueless. Of these, one was the delay in making them known; the other is the subject of this Note. A few words about the first may perhaps not be out of place.

The only pendulum observations which had obtained any repute were the following:—

1. 1670–1674, by Picard; at Paris, Uranibourg, La Haye, Lyons, and Cette;
2. 1671–1672, by Richer; at Cayenne and Paris;
3. 1681–1682, by Varin and Des Hayes; at Goree and Guadaloupe;
4. 1686, by Goüye; at Louveau (a place once the capital of Siam, but unknown to modern maps);
5. 1697–1698, by Couplet; at Paris, Lisbon, and Parayba;
6. 1699–1700, by Des Hayes; at Cayenne, Grenada, St. Christopher, and St. Domingo;
7. 1704–1705, by Feuillée; at Porto Bello, and Martinique.

Mention is sometimes made of a few others, but there are no authentic accounts; nor, indeed, are these, with one exception, of any present value except as historical indications of a certain kind. There is no trace (that I have met with) of any observation later than Feuillée's, until we come to De l'Isle's in 1728.

It is further noteworthy that in no case are the early observations described in detail, in a way to permit of any check upon the result. In general the observation consisted of lengthening or shortening the line by which a bullet or ball was suspended, until its swings were sensibly synchronous with those of a clock pendulum. The length is then recorded, and in most cases the length so determined is erroneous to an extent which can certainly not be attributable to error of observation in the ordinary sense. I attribute it only partly to large unsuspected variation of clock-rate during the diurnal period. Such variation is by no means incompatible with a fairly uniform daily rate; and that daily rates of a really startling uniformity *were usual* in those days is abundantly in evidence: see, for instance, Richer's observations at Cayenne, only sixteen years after the invention of pendulum clocks.

The account of De l'Isle's observations, given at considerable length, is therefore the first of its kind, and is especially interesting for that reason—at any rate to us; though, as I have said, the delay in publication no doubt made it sink into insignificance by the time it reached Paris, where in 1735 the elaborate observations of Mairan were no doubt occupying attention. And I think it probable that the interest which might have attached to it on account of the high latitude of the station was destroyed by the near expectation or actual possession shortly afterwards of those obtained by Maupertuis in 1737–8 at Pello and Paris.

But for these untoward conditions, I imagine the result obtained at Archangel would have received more notice. As it was, the French *savans* paid no attention to it; but it was considered by Krafft in his Essay on the Figure of the Earth, in 1736 (published 1741).*

I need hardly say it has been noticed by all modern writers. It was not, however, among the fifteen selected by Laplace, but it is included among the "second class" observations arranged by Airy, where its apparent error catches the eye as being much the largest. One of the objects of this Note is to rescue it from that invidious distinction.

To come to the point: the writer of the account has made a curious (but patent) blunder in deducing from the actual observation of *rate* the *linear* result which custom then and always has prescribed. I will quote his words:—

"Longitudinem penduli simplicis constat esse in ratione inuersa radicum quadratarum numeri oscillationum tempore æquali peractarum."

And he proceeds to apply this as follows. The length being

* Since this paper was written I have become acquainted with a very little known review of the early history of pendulum inquiry, viz. that of De Brémond, printed (as a note to his translation of the *Philosophical Transactions of the Royal Society*, for 1733 and 1734) in 1740. I hope to say more about this somewhat remarkable review elsewhere; for the present it will suffice to say that De Brémond falls into De l'Isle's error quite naturally.

440·5 lines, the pendulum made 28815 oscillations in 8 hours—*i.e.* in 28800 seconds. Therefore, the length must be reduced in the proportion of the *square root* of 28800 to the *square root* of 28815; whence a reduction of ·115 upon 440·5.

But the proportion should be as the *squares*, not as the *square roots*; and the reduction should therefore be four times as great.

Following out the calculation, and making the necessary arithmetical corrections, without introducing modern changes, I find: that the difference of length between Paris and Archangel, which the author determines as between the extremes ·163 and ·144 line, or about three-twentieths, should have been between ·507 and ·430, or nearly half a line.

But the result is better seen by noting that the seconds pendulum is determined as being between 441·007 and 440·930 lines, without reduction for atmospheric effects—say 440·97. Now, the allowance for air would be, with such a ball as was used on this occasion, about +·12, making the concluded length equal to 441·09.

It is natural now to inquire whether this result is in accordance with what we know of the Earth's surface. To test observations expressed in this way, I am in the habit of applying formulæ which assume

(1) That the seconds pendulum at the equator is 991 millimetres in length, with a symbolical correction.

(2) That the compression is $\frac{1}{290}$, with a symbolical correction.

(3) That "Clairaut's fraction" * is therefore ·00522, with a symbolical correction.

The length of the seconds pendulum in latitude λ will therefore be, in millimetres,

$$991(1 + \cdot00522 \sin^2 \lambda) + \text{symbolical terms.}$$

To find the length in inches or lines we must substitute 39·0161 or 439·306 for 991.

Now, the latitude of Archangel is 64° 32′; and therefore $\sin^2\lambda = \cdot8151$; whence we may find the length of the seconds pendulum in the latitude of Archangel to be—

$$439\cdot306 + 2\cdot2932 \times \cdot8151 = 441\cdot175 \text{ nearly.}$$

De l'Isle would seem, therefore, to have been only ·09 of a line in error, supposing my assumptions justifiable, and that there is no local disturbance at Archangel to account for it. A disturbance of that magnitude would be large, but not incredible.

I have already written at greater length on this not very

* This is the name given by Mr. Todhunter to the ratio of the increment of gravity from equator to pole to the force of gravity at the equator. The word 'therefore' in the text refers to Clairaut's Theorem.

important rectification than the occasion may seem to need. The truth is, that it has other aspects. It is, indeed, pleasant to rescue a good determination from an undeserved reproach; and it is curious to remark how the very obviousness of the mistake —like Poe's "Purloined Letter"—has allowed it to pass unnoticed. But it is rather as a justifying example of the need of sceptical criticism that I adduce it.

At the same time I would not be understood to hold the opinion that any gravimetrical observation that ever was made is valueless merely because it cannot compare with modern ones. The comparison is rather with what is possible according to experience. Suppose that a local disturbance of ·05 line (or 5^s per diem) is admissible: then why should we scorn an observation against which nothing can be said except that it ranges between ·03 and ·06 line, merely because so large a range is nowadays unusual? In the present case there is a more cogent objection, for not only is the range (*i.e.* the probable error) large, but also the mean denotes a disturbance which is greater than any others at present known *in that region*. All the same, it cannot be rejected summarily, like a great many others I could name, as being certainly quite wrong.

There is a work by the same author entitled *Histoire de l'Astronomie*, published at St. Petersburg in 2 vols. in 1738, which I believe reviews all the early observations with pendulums; but I have hitherto been unable to obtain it—neither of the libraries at the British Museum nor at Burlington House possessing the work—and I am curious to know whether the error above indicated is reproduced there.

Collingwood,
1880, *November* 25.

Saturn in October 1880. By W. F. Denning, Esq.

Towards the end of September and in October, when the planet came to opposition with the Sun, I made a number of observations of his physical appearance as revealed by a Browning—with reflector of 10 inches' clear aperture, powers 450 and 500, which I usually found best adapted to the work. The planet was carefully scrutinised on Sept. 21, 27, 30, Oct. 2, 11, 12, 14, 18, 20, 23, 29, 30, 31, Nov. 2, 3, and 4, and the appearances presented were generally the same, with this obvious difference, that on the nights of best definition more details were glimpsed than on less favourable occasions. My observations were usually conducted for five or six hours, during the whole of which attention was divided between the planets *Saturn* and *Jupiter*.

The telescopic aspect of *Saturn* may be thus described:—

I. *The Ball.*—The equatoreal zone is the brightest region of the disk, and gives indication of white patches in moments of

best definition. South of the equator there lies a faint, somewhat broad, dusky belt, in which no bright or dark spots are distinguishable; and next to this a slightly lighter interval, followed again by another belt, fainter than that just described, and displaying a very similar aspect. Another minute band of shading outlies this, and, further south, this is bounded by a very light zone, which contrasts greatly with the dusky bands. Where this bright belt comes up to the margin of the disk, it apparently projects beyond the limb, so as to give the impression of bright patches there, and causing a most striking distortion of the globe. Yet further south of the bright belt there come a series of very dark belts. The one immediately contiguous to the pole exhibits the greatest intensity of shading, and is, in fact, the darkest region of the disk, and there is a fainter band dividing this from the light belt. At the pole itself I at first suspected a bright cap, but renewed observations, made on several nights when the air was exceptionally steady and the planet free from the tremulous motion which so effectually destroys good definition, showed that the immediate pole, though lighter than the dark belt surrounding, was much enveloped in shade. North of the equator, and just skirting that part of the crape ring projected on the ball, is another faint belt, in which I can make out no variations of shading, nor are any distinct markings visible on the north segment of the ball, though it is much darker than the ring generally.

The divisions between the triple belt lying south of the equator, above described, were only observed on the best nights. As a rule, the belts were merged into one very broad band of shading, faintest at its south extremity.

II. *The Rings.*—The outer ring A is somewhat faint, especially at its extreme outer limits, and can be traced across the planet as a narrow bar of shading. Encke's division is not equally well observed at all times. Sometimes I see it as a pencil line only curving round the extremities of the major axis of the rings; but in the best moments it comes out plainer as a dark and distinct division. This was particularly noticed on Oct. 29 and Nov. 4. Ball's division was always very plain. The rings A and B where they skirt this division were much brighter than in other parts. On the ring A I frequently suspected faint lines of shading, but the slightest rippling or vibration prevented the observation of these delicate features, so that they never remained long enough in view to place their existence beyond a doubt. The brightest part of the system of *Saturn* is unquestionably the outer region of B, which is certainly brighter than the equatoreal light-zone. The interior part of B is shaded off until near the margin of the crape ring, where it apparenly recovers a little of the brightness which characterises its outer portions. Dawes and several other observers have represented the interior shading of B as gradual, but it always appears to me

as being very decided and abrupt, and to show a faint pencil-line where it begins. Further on its interior the depth of shading is greater, though the extreme inner border becomes lighter again, perhaps by contrast with the darkness of the crape ring. The crape ring C appears certainly brighter on its inner margin than where it adjoins the luminous vein of B, but this also may be due in a measure to the effects of contrast, for the inner edge of the ring is bounded by dark sky, while the outer margin is against the light rim of B. I have seen no certain division in the crape ring, but have recorded the extreme darkness of its outer border, which may amount to the same thing.

No spots or irregular markings were traced upon the ball or the rings, except, perhaps, some bright patches on the equatoreal region and at the limbs on the bright belt near the south pole. The belts, though extremely faint, could be traced nearly right up to the margin of the disk, in this case offering a distinct contrast to the belts of *Jupiter*, which are obliterated in coming up to the limb. Moreover, while the equator of *Saturn* is the brightest zone of the disk, that of *Jupiter* shows the greatest depth of shading.

The night of Oct. 29 was one of the best of the season. Between 10^h and 11^h the details of the planet were presented with magnificent distinctness. Powers up to 600 could be employed with effect, and even with 750 the view was fairly good, though I found that nothing was gained by the employment of anything higher than 450.

On Oct. 31 the belts were very plain between 7^h and 8^h—the one nearest equator evidently darker than the others; and there were varieties in tone near the margin.

On Sept. 21 the dark space between the ball and crape ring looked larger on the east than west ansa, and slight differences were noticed on other nights, though not always of the same character.

But by far the most striking feature in the telescopic appearance of *Saturn* was the *curious figure* of the globe. The south pole, over which the dark belts lay, seemed *compressed* in the most remarkable manner; and where the bright belt intervened, the contrary effect was produced. Here the limb was apparently raised above the circular contour, so that the distorted image gave the planet that distinctly "square-shouldered" aspect which has been occasionally mentioned as a singular anomaly in observations of Saturn. The figure was on several recent occasions, when definition could hardly be improved, so clearly manifested, that the planet could not have been attributed a truly spherical form unless the observer had known it to have been unequivocally demonstrated by micrometric observations.

The explanation appears to me very simple. The singular figure is due to the contrasting effects of the belts. While the bright belt near the pole causes a very evident "shouldering" out of the two limbs at its extremities, the dark belts at the pole and

towards the equator act in a precisely contrary way. They apparently compress the planet where they come up to the limb, and thus the distorted figure is originated. I tried to get rid of this by very careful focussing, and thought that in transient intervals of superb definition the true spheroidal form of the planet would come out; but this was not the case. The singularity of figure was constantly maintained; so as, in fact, to form one of the most prominent features in the telescopic aspect of the planet; though it was not always equally decided, for at the end of October and on the first few nights of November it was not nearly so conspicuous as during some of the earlier observations.

Observations of the Great Southern Comet I. (1880) *made at Castlemaine, Victoria.* By William Bone, Esq., M.D. (*Communicated by the Secretaries.*)

I take the liberty of forwarding a memorandum of my discovery of the Comet of February last, together with my measurements.

I gave Mr. R. A. Proctor a rough memorandum of the same when here recently; but since then I have taken a fresh series of observations for ascertaining truthfully my *micrometer values*, which has altered the interpretation of the observations in declination.

For the care and accuracy of the actual measures I can now with confidence vouch, having waited until the stars came round again, and verified every position. I offer them as an amateur's effort.

1880. Feb. 2. Discovered.—At 7^h 30^m on the evening of Feb. 2 last, on going to my Observatory (in company with the Rev. E. Crawford, Church of England priest of this town), I noticed a peculiar band of light near the position of the Sun, which had recently set. At first I thought it was a shoot of light from one of the foundry furnaces, but its stability soon convinced me it could only be the tail of a Comet. As the light of day waned, this was quite apparent. The nucleus was below the horizon, and must have been very close to the Sun. The tail extended obliquely upwards for about 35°, near β *Gruis* and towards *Nebula Minor*. A careful watch was kept from this time. I then telegraphed to Mr. Ellery at the Melbourne Observatory, and sent daily reports as long as the Comet was in view; but he was not fortunate enough to see the nucleus for several days after I had measured its position. There was no sign of the Comet before sunrise on the following morning. My little Observatory was besieged.

Feb. 3.—The Comet's nucleus was still below the horizon.

The tail was a little S. of S.W., and had perceptibly travelled East and North. It was not so bright as on the previous evening, and from that circumstance and its receding from the Sun, I surmised it had passed its perihelion point.

Feb 4. Nucleus first seen.—On this evening the sky was partially obscured by a few loose cumulo-strati. At $8^h\ 45^m$ G. M. T. they cleared off, and I had about ten minutes' distinct view of the nucleus. It was near the horizon, and an intervening building soon destroyed my view. During the short view I had I could clearly see a "vacuole" and the reflected (to use an anatomical term) envelopes, similar to the drawings of Donati's Comet.

Feb. 5.—The approximate positions shown by the Circles of the Equatoreal (Z. Gaunt, Melbourne, with $4\frac{3}{4}$-inch Wray object-glass) were—

	h	m	s		°	′	″
R. A.	22	28	0,	Decl. S.	33	35	0,

	h	m	s
Time	7	30	0.

Fearing to lose my view speedily, I had arranged an Altazimuth on the top of a hill near, 200 feet above my level, and the position of the Comet's nucleus at $8^h\ 58^m$ was *exactly four degrees* in azimuth south of *Fomalhaut* and between 1820 and 1816 La Caille *Piscis Australis*, having *the same Right Ascension exactly as* 1820.

Feb. 6.—The nucleus was again seen, but when found was too low down for the Equatoreal.

Feb. 7 and 8.—Sky overcast. No observations.

Feb. 9.—Clear. Micrometric observations were obtained from μ *Sculptoris*, the Circles giving Comet's approximate place—

	h	m	s		°	′	″
R. A.	23	37	11,	Decl. S.	33	30	0,

	h	m	s	
Mean of Times of Measurement	9	2	30	G. M. T.
Difference in R. A.: Star preceding	0	1	35·272,	

	°	′	″
Difference in Declination: Comet south of Star	0	6	20·625.

The nucleus was fainter and smaller, and the Comet was evidently receding rapidly. Great care had to be taken in illuminating the webs, as it was so faint. The measurements given are, however, reliable, the greatest possible care having been taken. The micrometer was a Ramsden's, by Ross, of London.

Feb. 10.—The nucleus was again seen, but my professional engagements had detained me, and before any measurements could be taken it became overcast. The approximate position given by the Circles was*—

R. A. $23^h\ 55^m\ 27^s$, Dec. S. $33^\circ\ 28'\ 0''$.

Feb. 11, 12, 13.—Overcast.

Feb. 14.—Good measurements were obtained from σ *Sculptoris*.

The approximate place of the Comet by the Circles was—

R. A. $1^h\ 2^m\ 58^s$, Dec. S. $32^\circ\ 3'\ 0''$.

The differences by means of *seven* measurements at a mean of time $9^h\ 31^m\ 15^s$ Greenwich mean time were—

Difference in R. A.: Star preceding $+0^h\ 5^m\ 1{\cdot}16^s$,

Difference in Declination: Comet south of Star $+0^\circ\ 6'\ 35{\cdot}85''$.

The night was beautifully clear, and these were capital observations. I applied the spectroscope, but its light was too faint with my aperture to get any reliable reaction.

Feb. 15.—Splendid measurements were obtained from two stars nearly same R. A. and about 14′ apart, the Comet showing between them. [Sketch annexed.] Approximate position of Comet was—

R. A. $1^h\ 12^m\ 14{\cdot}35^s$, Decl. S. $31^\circ\ 33'\ 0''$.

The southernmost star I will call A, the northern B. They were about 5 and 6 mag., as far as I could estimate, but having no larger star catalogue than that contained in "Loomis," I am not able to identify them. At first I thought they were τ and π *Sculptoris*, but the distance between them is not sufficient. I have C. Dieu and Flammarion's maps, and there are marked stars corresponding to these in R. A. but in Decl. S. 34° 0′ 0″. I know my telescope is within a minute, and is so correct that I can find stars in that region easily from catalogue places without altering the setting of the verniers. There is no error in the sixth correction.

* On this day I fancied the Comet was affected by the attraction of *Jupiter*.

		h m s
Means of Times =		9 20 11 G. M. T,
Difference in R. A. Comet preceding,	from A,	−0 2 5·2,
	from B,	−0 2 17·6,
		° ′ ″
Difference in Decl.	from A, Star north,	0 6 35·719,
	from B, Star south,	0 7 26·25.

These were also beautiful observations.

Feb. 16.—Again I had beautiful measurements. [Sketch annexed.] There is a group similar to this in Flammarion's map, but in different Declination to my reading, but there is also another group marked in correct position very similar. The above is a faithful sketch of the appearance at $10^h\ 16^m\ 19^s$ Greenwich mean time. The approximate position of the Comet by the Circles was—

	h m s
R. A.	1 23 55,
	° ′ ″
Dec. S.	30 57 0,
Difference in R. A.: Star preceding,	+ 0 3 8·264
Difference in Declination: Comet south of Star,	+ 0 5 5·288.

Feb. 17.—The nucleus was visible after a long search, but was too faint to bear illumination, so that measurements could not be obtained. The approximate position by the Circles was—

h m s		° ′ ″
R. A. 1 41 22·5	Decl. S.	30 2 0

After this "he went his way and I saw him no more," and I much fear me I never shall again.

Latitude S. 37° 4′ 10″·97. Longitude E. $9^h\ 36^m\ 56^s$.

Castlemaine, Victoria, Australia,
1880, *October* 10.